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# Nonpersistent Inequality in Educational Attainment: Evidence from Eight European Countries<sup>1</sup>

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In their widely cited study, Shavit and Blossfeld report stability of socioeconomic inequalities in educational attainment over much of the 20th century in 11 out of 13 countries. This article outlines reasons why one might expect to find declining class inequalities in educational attainment, and, using a large data set, the authors analyze educational inequality among cohorts born in the first two-thirds of the 20th century in eight European countries. They find, as expected, a widespread decline in educational inequality between students coming from different social origins. Their results are robust to other possible choices of method and variables, and the authors offer some explanations of why their findings contradict Shavit and Blossfeld's conclusions.

## INTRODUCTION

In their seminal study on the development of inequality in educational attainment in the 20th century, Shavit and Blossfeld (1993) summarize

<sup>1</sup> Earlier versions of this article were presented at the Start-Up Workshop of the EDUC Research Theme of the Sixth EU Framework Network of Excellence, "Economic Change, Quality of Life and Social Cohesion (EQUALSOC)," Mannheim, December 2–3, 2005; the meeting of Research Committee 28 (ISA) Inequality and Mobility in Family, School, and Work, Los Angeles, August 18–21, 2005; a meeting of the Inter-university Working Group on Social Inequality and Life Course, Utrecht, June 22, 2005; and the Euresco conference "European Society or European Societies? Euro-Conference on the Causes and Consequences of Low Education in Contemporary Europe," Granada, September 18–23, 2004. We are grateful for comments and suggestions made at these meetings. The data used in this article were kindly made

the results under the guiding title *Persistent Inequality*. In spite of dramatic educational expansion during the 20th century, of the 13 countries studied in their project, all but two, Sweden and the Netherlands, “exhibit stability of socio-economic inequalities of educational opportunities. Thus, whereas the proportions of all social classes attending all educational levels have increased, the relative advantage associated with privileged origins persists in all but two of the thirteen societies” (p. 22). This conclusion is based on a metanalysis of individual country studies, all of which adopt two different approaches to assess socioeconomic inequalities of educational opportunities: one is to use ordinary least squares to regress years of education achieved by sons and daughters on parents’ education and occupational prestige; the other is to regress, using binary logistic regression, a set of successive educational transitions on the same social background variables (the “Mare model”; Mare 1980, 1981). Change or persistence in inequalities of educational opportunities is diagnosed depending on whether or not significant variation over birth cohorts is found in the regression coefficients linking social background to years of education attained and the educational transitions considered. While the two analyses address different empirical phenomena—of which Shavit and Blossfeld are well aware—the results of both suggest essentially the same conclusion, which the authors then summarize as “stability of socio-economic inequalities of educational opportunities.” In the scientific community, in particular in sociology and in the education sciences, the results have been viewed as evidence of a persistently high degree of class inequality of educational attainment that can change only under rather exceptional conditions.

Shavit and Blossfeld’s result echoed earlier findings from some single-country studies,<sup>2</sup> but subsequently several analyses have contested this finding. They have shown that equalization also took place in Germany (Müller and Haun 1994; Henz and Maas 1995; Jonsson, Mills, and Müller 1996), France (Vallet 2004), Italy (Shavit and Westerbeek 1998), and the United States (Kuo and Hauser 1995). Rijken’s (1999) comparative analysis comes to the same conclusion. In other studies, Breen and Whelan

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<sup>2</sup> For Britain, Halsey, Heath, and Ridge (1980); for the United States, Featherman and Hauser (1978); for France, Garnier and Raffalovich (1984); and for the Netherlands, Dronkers (1983).

(1993) and Whelan and Layte (2002) confirm persistent inequality for Ireland, whereas for Soviet Russia, Gerber and Hout (1995) find mixed results (declining inequality in secondary education and increasing inequality in transitions to university). For the postsocialist period in various countries of Eastern Europe, the origin-education association is regularly found to be very high and is likely higher than in the socialist period (Gerber [2000] for Russia; Iannelli [2003] for Hungary, Romania, and Slovakia).

The aim of this article is to reassess the empirical evidence concerning the conclusion of *Persistent Inequality* using more recent data and larger samples from a selection of European countries. In contrast to Shavit and Blossfeld, we base our conclusions on analyses using ordered logit models of educational attainment rather than on educational transition models. The reason is that we are interested in inequalities related to social origin in completed education, which constitutes the major starting condition for unequal opportunities in the life course. Another reason for not using educational transition models is that we lack data on individuals' complete educational histories. Indeed, there are no cross-nationally comparable large data sets that contain complete education histories and also cover long historical periods. In the absence of information on educational careers, researchers often have assumed that their subjects have pursued the most typical paths and have then constructed hypothetical transition patterns from the observed highest level of education. But, particularly for countries with a highly differentiated educational system (most European countries, in fact), such constructions must give a seriously distorted picture of the real patterns of educational transitions (Breen and Jonsson 2000).

Our results show that there was a clear decline in educational inequality in several countries over the course of the 20th century. This inevitably raises the question of why we arrive at such a different conclusion from that of Shavit and Blossfeld. As we explain below, we believe that there is a strong *prima facie* case for expecting decline rather than constancy in educational inequality. But we also seek to assess the degree to which our results might be sensitive to questions of method. Ideally we would have liked to replicate Shavit and Blossfeld's analyses, but this is not feasible. The chapters in their volume are, in fact, quite heterogeneous in both their explanatory and dependent variables. Social origins are measured in all countries in terms of parental education (though in some cases this is years of education, in others the highest level of education attained) and either parental socioeconomic status (using a variety of scales such as Wegener's magnitude prestige scale, Treiman's occupational prestige scale, and the Hope-Goldthorpe scale) or categorical social class. "Parental" in some cases means the father; in others it means the parent with

the higher education and more “dominant” class position. Furthermore, although all the analyses employ the Mare model of educational transitions, the educational categories, and thus the transitions themselves, are defined in different ways, which is a consequence of not adopting a common educational classification across the 13 countries studied.

Such variety not only makes cross-national comparisons impossible (as Shavit and Blossfeld recognize), but also vitiates any attempts to compare our approach with theirs, for the simple reason that they do not have a single approach. In contrast, we compare trends within (and, as we discuss later, between) eight European countries using a common educational categorization and, with relatively minor exceptions, a common schema of class origins. Our methodological investigations take the form of varying the analytical model (by also employing the Mare model), introducing parental education as an additional indicator of social background, considering differences in the birth cohorts and periods covered in the data, and varying the sample size to match that of the country analyses in Shavit and Blossfeld’s volume. The object of all this is to investigate the robustness of our results and, in the process, to shed some light on the question of why the analyses of their contributing authors led Shavit and Blossfeld to conclusions different from ours.

In the following sections of the article we begin with a discussion of why we think educational inequality should have declined during the 20th century. This concentrates on the broad pattern of developments, around which, of course, specific countries showed some variation. We then present our data and our results concerning the evolution of educational inequality in eight European countries. We next turn to methodological concerns and assess the robustness of what we have found to different ways of analyzing our data. In the conclusion we return to our substantive finding of equalization of educational inequality, we compare the extent of educational inequality between countries, and we discuss the implications of our findings.

#### REASONS TO SUPPOSE THAT EDUCATIONAL INEQUALITY WAS NOT PERSISTENT

Differences between students from different social classes in how they fare in the educational system can, in simple terms, be seen to derive from differences in how they perform in the educational system (which Boudon [1974] called “primary effects”) and differences in the educational choices they make, even given the same level of performance (“secondary effects”). In both areas, developments in the course of the 20th century would lead us to expect declining class differences.

As far as primary effects are concerned, children raised in families in the more advantaged classes encounter better conditions in their home environments that help them to do better in school. They get more intellectual stimulation that strengthens their cognitive abilities, and their parents are more highly motivated and supportive of schoolwork than parents of working-class children. Different performance at school may also derive from different nutrition and health in different classes, whereas genetic differences between individuals from different class backgrounds may play a role as well as class differences in sibship sizes (see Erikson and Jonsson 1996a). Yet, as Erikson and Jonsson (1996b, p. 81) suggest, the general improvement in conditions of living should have made working-class children less disadvantaged in terms of health and nutrition. With economic development and welfare-state protection, the minimum standards of living have improved and average family size has declined. Such changes should have been more relevant for families in the less advantaged classes, such as the working classes and the small-farmer class, who have been able to move out of absolute economic misery. Some decline in primary class effects should thus have occurred over the long term and particularly during the substantial improvement of general living conditions in the decades of economic growth and welfare-state expansion following World War II. This should have been reinforced by changes within educational institutions, such as the growth in public provision of early child care and preschool education; the development of full-day rather than part-time schooling; increased school support to counteract performance gaps of pupils; and differences in the timing, extent, and manner of tracking, all of which may reduce class differences in school performance.

As far as secondary effects are concerned, one factor that should have brought about a major reduction is the declining costs of education. Direct costs, especially in secondary education, have become smaller; school fees have been largely abolished; the number of schools has increased, even in rural areas, so schools can be reached more easily; and traveling conditions have improved. In many countries, educational support programs for less wealthy families have been set up, albeit of rather different kinds and levels of generosity. Real average family income has increased, and that should make it easier to bear the costs of education. While at least in the first half of the last century working-class children were urged to contribute to the family income as early as possible, such pressures have declined. In most countries, economic growth and the reduction in family size have led to an increase in disposable incomes beyond what is required for basic needs. In practically all countries the length of compulsory schooling has expanded, thus reducing the number of additional school years beyond compulsory education needed to reach full secondary ed-

ucation. Countries certainly differ in the specifics of institutional reforms, and these probably have different implications; but the lengthening of compulsory education should everywhere have contributed to a decline in the additional costs of postcompulsory education.

Countries also differ in their welfare-state and social security arrangements and in their ability to prevent unemployment among students' parents. In countries such as Sweden, in which serious income equalization policies have been pursued successfully, the equalization of conditions is believed to have had an additional impact on reducing the class differential in the ability to bear the costs of education. The recurrence of high levels of unemployment in many countries since the 1980s, especially for the unskilled working class, and the increase in income inequality observed in some countries in recent years (Alderson and Nielsen 2002) are probably the most important changes that may have counteracted a long-term trend toward lowering the impact of costs in producing class inequalities in educational participation, but these developments are mostly too recent to be evident in our data. In sum, both primary and secondary effects changed in ways such that declining disparities between classes in educational attainment can be expected; in particular, it is the children of working-class and farm families who should have most markedly improved their relative position.

#### DATA

Our data come from nine European countries—Germany, France, Italy, Ireland, Britain, Sweden, Poland, Hungary, and the Netherlands—and they were originally assembled for a comparative analysis of social mobility in Europe (Breen 2004). That project sought to bring together all the high-quality data sets collected between 1970 and 2000 in 11 European countries that could be used for the analysis of social mobility. The data used here are identical to those employed in that project except that the German data have been augmented by six surveys. These six surveys contain the first three German Life History Surveys for West Germany (fielded between 1981 and 1989) as well as the 2000 sample for West Germany from the German Socio-economic Panel and the ALLBUS Surveys for 2000 and 2002. The Hungarian data are excluded from the final analysis, as will be described later. The data sets that we use are listed in table 1.<sup>3</sup> In total we use 120 surveys collected between 1970 and 2002,

<sup>3</sup> Of the countries in the original study we do not include two: Norway, because of problems with the coding of class origins, which led to the Norwegian data's exclusion from most of the comparative analyses in Breen and Luijkx (2004a, 2004b), and Israel, because we lack information on educational attainment in one of the two surveys used by Yaish (2004).

but each country provides rather different numbers of surveys (up to a maximum of 35 from the Netherlands). In Sweden, for example, there is a survey for every year from 1976 to 1999, whereas the analyses for Italy are based on only two surveys and those for Ireland and Poland on only three surveys each.

We use data on men ages 30–69 (30–59 in Great Britain, except for the years 1979–88, when the age range is 30–49). We adopt 30 as the lower age limit to ensure that everyone in the samples will have attained his highest level of education, and we take 69 as an upper limit in order to minimize any effects of differential mortality.<sup>4</sup> We confine our analysis to men because the inclusion of both sexes, and comparisons between them, would have made a long article excessively so. We intend to analyze educational inequality among women, and compare it with the results reported here, in a further paper.

#### VARIABLES

We use four variables in our analysis. *Cohort* (*C*) defines five birth cohorts: 1908–24, 1925–34, 1935–44, 1945–54, and 1955–64. Thus we have information on cohorts born throughout the first two-thirds of the 20th century. *Survey period* (*S*) defines the five-year interval in which the data were collected: 1970–74, 1975–79, 1980–84, 1985–89, 1990–94, 1995–99, and 2000–2004.

*Highest level of educational attainment* (*E*) is measured using the CAS-MIN educational schema (Comparative Analysis of Social Mobility in Industrial Nations; see app. table A1 below; Braun and Müller 1997). We have amalgamated categories 1a, 1b, and 1c and also 2a and 2b, giving us five educational categories:<sup>5</sup>

1abc.—Compulsory education with or without elementary vocational education

<sup>4</sup> In any event, it is far from clear what impact differential mortality might have on conclusions concerning trends in educational inequality. If, for instance, lower-educated individuals tended to die younger, this would have no impact on our conclusions. But if mortality were selective according to education and class origins, such that lower-educated people from lower social class origins had shorter life expectancy, this would tend to understate the extent of class inequality in older cohorts and would lead us to underestimate the decline in inequality over cohorts.

<sup>5</sup> Higher tertiary education, 3b, means the successful completion (with examination) of a traditional, academically oriented university education. Lower tertiary education, 3a, is usually characterized by a shorter length of study and more practically oriented study programs (e.g., technical college diplomas, social worker, or nonuniversity teaching certificates).



TABLE 1  
SOURCES OF DATA

Country	No. of Tables	Sources of Data	Years for Which Data Are Included
Germany .....	30	Zumabus ALLBUS  Politik in der BRD Wohlfahrtssurvey German Life History Study German Socio-economic Panel Formation-qualification professionnelle INSEE surveys	1976-77, 1979 (2), 1980, 1982 1980, 1982, 1984, 1986, 1988, 1990-92, 1994, 1996, 1998, 2000, 2002 1978, 1980 1978 1981-83 (I), 1985-88 (II), 1988-89 (III) 1986, 1999, 2000 1970, 1977, 1985, 1993
France .....	4		
Italy .....	2	National Survey on Social Mobility Italian Household Longitudinal Survey	1985 1997
Ireland .....	3	Survey of the Determinants of Occupational Status and Mobility Survey of Income Distribution and Poverty Living in Ireland Survey	1973  1987 1994
Great Britain ...	15	General Household Survey	1973, 1975-76, 1979-84, 1987-92
Sweden .....	24	Annual Surveys of Living Conditions (ULF)	1976-99
Poland .....	3	Zagórski (1976) Slomczynski et al. (1989)	1972 1988
		Social Stratification in Eastern Europe after 1989	1994
Hungary .....	4	Social Mobility and Life History Survey Way of Life and Time Use Survey (Hungarian Central Statistical Office)	1973, 1983, 1992 2000

Netherlands .....	35	Parliamentary Election Study	1970, 1971, 1977, 1981, 1982, 1986, 1994, 1998
		Political Action Survey I	1974, 1979
		Justice of Income Survey	1976
		CBS Life Situation Survey	1977, 1986
		National Labour Market Survey	1982
		National Prestige and Mobility Survey	1982
		Strategic Labour Market Survey	1985, 1988, 1990, 1992, 1994, 1996, 1998
		Cultural Changes (ISSP)	1987
		Justice of Income Survey	1987
		Primary and Social Relationships	1987
		Social and Cultural Trends	1990
		Justice of Income Survey (ISJP)	1991
		Family Survey I, 1992-93	1992
		Households in the Netherlands pilot	1994
		Households in the Netherlands	1995
		Social Inequality in the Netherlands	1996
		National Crime Study	1996
		Social and Economic Attitudes	1998
		Netherlands Family Survey II	1998
		Use of Information Technology	1999

- 2ab.—Secondary intermediate education, vocational or general
- 2c.—Full secondary education
- 3a.—Lower tertiary education
- 3b.—Higher tertiary education

We have only four educational categories in the Hungarian data, where 2ab is missing, and in the Italian and the Irish data, where no distinction has been made between 3a and 3b. The CASMIN educational schema seeks to capture distinctions not only in the level of education but also in the type, and one consequence of this is that the five levels we identify cannot be considered to be sequentially ordered in any simple way. For example, in some countries lower tertiary education can be accessed directly from secondary intermediate education, whereas in most countries, higher tertiary is not usually entered after lower tertiary.

*Class origins (O)* are categorized using the Erikson-Goldthorpe-Portocarero (EGP) class schema (see app. table A2; also Erikson and Goldthorpe 1992, chap. 2). We identify seven classes:

- I.—Upper service
- II.—Lower service
- IIIa.—Higher-grade routine nonmanual
- IVab.—Self-employed and small employers
- IVc.—Farmers
- V+VI.—Skilled manual workers, technicians, and supervisors
- VIIab+IIIb.—Semi- and unskilled manual, agricultural, and lower-grade routine nonmanual workers

In Britain and Poland the data allow us to identify only six class origins. In both countries we cannot distinguish classes I and II, whereas in Britain, members of IVa are included in I+II (see Goldthorpe and Mills 2004). Furthermore, in Poland, we cannot split class III, so here IIIb is included with IIIa rather than with VIIab. In Ireland also we combine I and II because of very small numbers in class I in some cohorts.

The resulting four-way table of class origins (*O*) by educational attainment (*E*) by cohort (*C*) by survey period (*S*) is of maximum dimensions  $7 \times 5 \times 5 \times 7 = 1,225$ , though this number includes many structural zeros in those combinations of cohort and survey that are not observed. Furthermore, we omitted all those observations of cohort by survey in

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TABLE 2  
SAMPLE SIZES FOR COHORTS BY COUNTRY

COUNTRY	COHORT					TOTAL
	1908–24	1925–34	1935–44	1945–54	1955–64	
Germany .....	2,323	3,110	4,852	4,007	2,832	17,124
France .....	11,283	14,169	13,126	10,517	2,610	51,705
Italy .....		742	1,182	1,286	827	4,037
Ireland .....	744	1,176	1,509	1,289	863	5,581
Great Britain ...	6,473	9,112	24,481	20,971	5,437	66,474
Sweden .....	8,032	8,209	10,875	10,145	4,093	41,354
Poland .....	7,729	9,248	8,851	1,002	784	27,614
Hungary .....	5,698	6,829	7,175	5,643	2,583	27,928
Netherlands ....	2,385	3,507	4,736	5,612	3,511	19,751
Total .....	44,667	56,102	76,787	60,472	23,540	261,568

which the table of origins by education would have been extremely sparse. All the cells in such a table were treated as structural zeros.<sup>6</sup>

Table 2 shows the resulting sample sizes for all the countries by cohort. These vary quite considerably, and this will obviously affect our ability to detect statistically significant trends. The sample sizes for Italy and Ireland are particularly small, and one consequence of this is that we have very few observations of the oldest cohort in Italy, so we omit it from our analyses.

## CHANGES IN EDUCATIONAL ATTAINMENT AND CLASS ORIGINS

Perhaps the single most striking thing that differentiates the older from the younger cohorts in our data is the massive increase in educational attainment that has occurred. Figure 1 shows the proportions in each cohort in each country that have attained at least upper-secondary (2c) education, and figure 2 shows the proportions having attained tertiary (3a and 3b) education.<sup>7</sup> The upward trends in both are obvious and are similar across countries.

It is not only the educational distributions that have shifted, however. During the course of the 20th century the class structures of European nations underwent major change, with a move away from farming and unskilled occupations toward skilled jobs and white-collar jobs. Some

<sup>6</sup> Because of heavy losses in World War II and the consequent small number of cases, we did not include respondents born before 1915 in Germany's first cohort. Germany's first cohort thus includes only respondents born 1915–24.

<sup>7</sup> At this point we omit Hungary from our comparison: the reasons for this are given in the next section of the article.

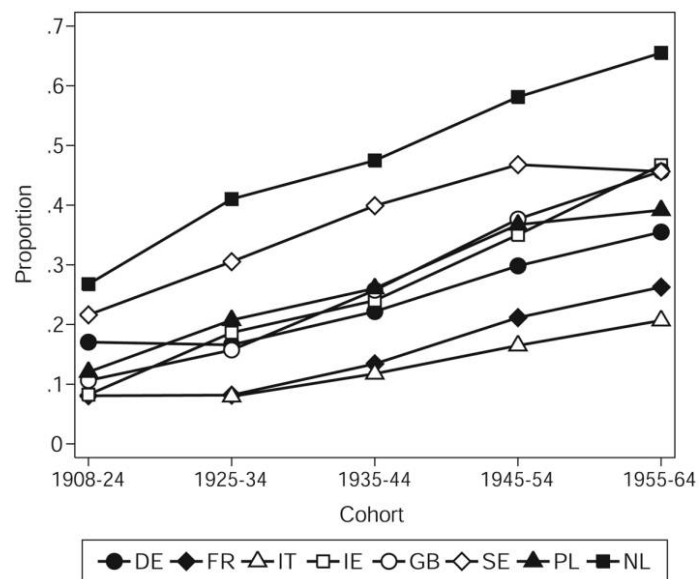


FIG. 1.—Proportion of men reaching at least upper-secondary education, by country and cohort.

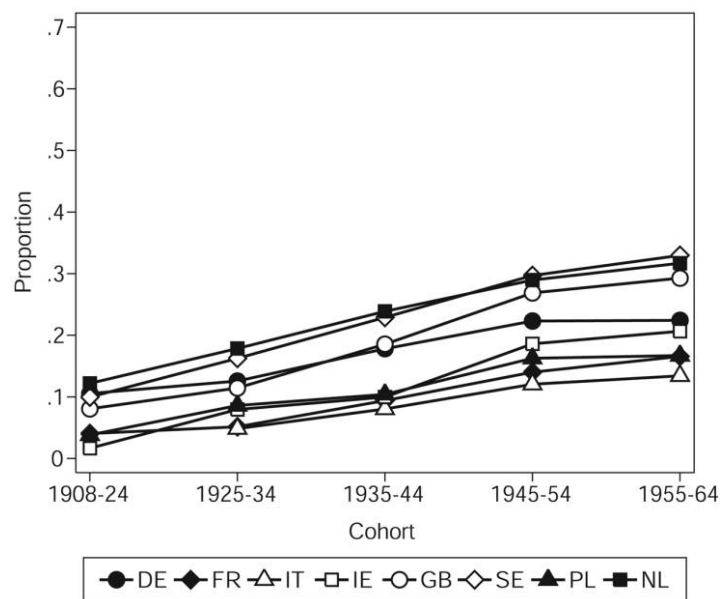


FIG. 2.—Proportion of men reaching tertiary education, by country and cohort

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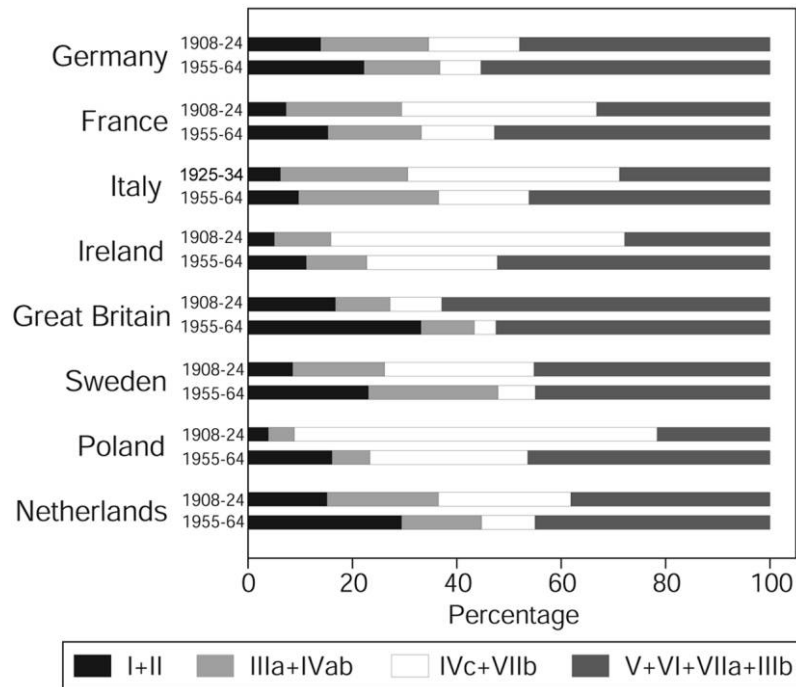


FIG. 3.—Proportions of men in various class origins for first and last cohort by country (marginal distributions). In Great Britain, class IVa is included in classes I+II; in Poland, class IIIb is included in classes IIIa+IVab.

aspects of this are shown in figure 3, which reports the share of the service class (I and II), intermediate class (IIIa and IVab), the farm classes (IVc and VIIb), and working class (V+VI, VIIa+IIIb) in the origins of the oldest and youngest cohorts in each country. The decline of the farm class and growth of the service class are evident everywhere, and the working class has grown or remained stable everywhere except Britain.

## ANALYSES

We carry out two sets of analyses, the first of which attempts to assess the quality of our data. We then turn to the main object of the article and model the trend over cohorts in the strength of the association between class origins and highest level of educational attainment (educational inequality, in other words).

Assessing the Quality of the Data

The structure of our data and the choice of the age range of 30–69 for our samples allow us to assess the quality of our data in a way that is not normally possible. Among any sample of adults, their class origins are fixed, and the acquisition of further educational qualifications after the age of 30 is rare. As a result, if we had longitudinal data, we could assess the reliability of measures of class origins and of educational attainment by comparing individuals' responses at different points in time (a strategy that was used by Breen and Jonsson [1997] to measure reliability in reports of class origins). In our case we have repeated cross-sections rather than longitudinal data, and so different surveys will contain samples from the same birth cohort. Under certain conditions,<sup>8</sup> we should expect that variation between surveys within the same birth cohort in the distribution of education and class origins should not exceed what we would expect on the basis of sampling variability. Thus we can use the fact that our data consist of samples from the same cohorts at different periods to check (a) whether the marginal distributions of education and class origins remain constant, within the limits of sampling variability; and (more important from our point of view) (b) whether the association between these variables is also constant.

To do this we fit four log-linear models to the four-way tables of class origins (*O*) by educational attainment (*E*) by cohort (*C*) by survey period (*S*) in each of our nine countries, and the results are shown in table 3. For each country there are four models, all of which allow the origin-education association to vary over cohorts (they all include the *COE* term) and also allow the distribution of cohorts to vary over surveys (the *CS* term). Our interest is in whether the *CO*, *CE*, and *OE* relationships vary over surveys. In model 1 none of them does; in model 2 the distribution of class origins in each cohort, *CO*, is allowed to differ over surveys, yielding the term *CSO*; in model 3, so is the distribution of education (*CE*), giving *CSE*; and in model 4 we allow the association between origins and education (*OE*) to vary over surveys (*SOE*).<sup>9</sup> We assess the goodness

<sup>8</sup> The most important condition is that the samples should be drawn from the same population. This condition could be violated by such period influences as mortality (though, given our age range, this is unlikely to be important) and by migration. Over the last decades of the 20th century, Ireland, e.g., experienced substantial immigration and emigration, and several other countries have experienced immigration. Another condition is that the various surveys in a country should sample the population in the same way. As we noted earlier, in many of our countries (France, Britain, Hungary, and Sweden) the data are drawn from the same survey series, but this is not true of Germany, Italy, Ireland, Poland, and the Netherlands.

<sup>9</sup> This means that the origin-education association differs across surveys *within* cohorts, but this survey effect is the same in all cohorts.

TABLE 3  
TEST OF HOMOGENEITY WITHIN COHORTS: MODELS FITTED TO ORIGIN-BY-  
EDUCATION-BY-COHORT-BY-SURVEY TABLES

					CONDITIONAL TESTS			
	$G^2$	$df$	$P$	$\Delta$ (%)	Models	$G^2$	$df$	$P$
Germany:								
1. $COE\ CS\ SO\ SE$	623.5	528	.003	6.17				
2. $COE\ CSO\ SE$	524.7	456	.014	5.08	1-2	98.8	72	.020
3. $COE\ CSO\ CSE$	455.3	408	.053	4.58	2-3	69.4	48	.023
4. $COE\ CSO\ CSE\ SOE$	301.9	288	.275	3.63	3-4	153.4	120	.021
France:								
1. $COE\ CS\ SO\ SE$	455.7	344	.000	2.56				
2. $COE\ CSO\ SE$	370.6	296	.002	1.99	1-2	85.1	48	.001
3. $COE\ CSO\ CSE$	304.4	264	.044	1.83	2-3	66.2	32	.000
4. $COE\ CSO\ CSE\ SOE$	196.0	192	.406	1.42	3-4	108.4	72	.004
Italy (4 cohorts, 4 educational categories):								
1. $COE\ CS\ SO\ SE$	79.2	72	.262	3.84				
2. $COE\ CSO\ SE$	66.1	60	.275	3.10	1-2	13.1	12	.362
3. $COE\ CSO\ CSE$	62.7	54	.195	2.91	2-3	3.4	6	.757
4. $COE\ CSO\ CSE\ SOE$	39.9	36	.302	1.98	3-4	22.8	18	.198
Ireland (4 educational categories, 6 classes):								
1. $COE\ CS\ SO\ SE$	154.1	122	.026	4.59				
2. $COE\ CSO\ SE$	116.4	102	.156	3.87	1-2	37.7	20	.010
3. $COE\ CSO\ CSE$	102.7	90	.170	3.62	2-3	13.7	12	.320
4. $COE\ CSO\ CSE\ SOE$	62.2	60	.398	2.71	3-4	40.5	30	.096
Great Britain (6 classes):								
1. $COE\ CS\ SO\ SE$	350.9	283	.004	2.16				
2. $COE\ CSO\ SE$	299.1	248	.015	1.95	1-2	51.8	35	.034
3. $COE\ CSO\ CSE$	246.7	220	.105	1.69	2-3	52.4	28	.003
4. $COE\ CSO\ CSE\ SOE$	131.4	140	.686	1.15	3-4	115.3	80	.006
Sweden:								
1. $COE\ CS\ SO\ SE$	576.6	504	.014	3.98				
2. $COE\ CSO\ SE$	479.0	432	.058	3.44	1-2	97.6	72	.024
3. $COE\ CSO\ CSE$	390.3	384	.402	3.00	2-3	88.8	48	.000
4. $COE\ CSO\ CSE\ SOE$	271.4	288	.751	2.47	3-4	118.9	96	.057
Poland (4 educational categories, 6 classes):								
1. $COE\ CS\ SO\ SE$	113.8	99	.146	.83				
2. $COE\ CSO\ SE$	98.1	84	.140	.72	1-2	15.8	15	.398
3. $COE\ CSO\ CSE$	84.6	75	.209	.60	2-3	13.4	9	.144
4. $COE\ CSO\ CSE\ SOE$	62.6	45	.042	.49	3-4	22.0	30	.853
Hungary (4 educational categories):								
1. $COE\ CS\ SO\ SE$	424.5	243	.000	3.37				
2. $COE\ CSO\ SE$	350.6	201	.000	2.61	1-2	73.9	42	.002
3. $COE\ CSO\ CSE$	306.1	180	.000	2.37	2-3	44.5	21	.002
4. $COE\ CSO\ CSE\ SOE$	159.9	126	.022	1.39	3-4	146.2	54	.000



TABLE 3 (Continued)

					CONDITIONAL TESTS			
					Models	$G^2$	$df$	$P$
Netherlands:								
1. <i>COE CS SO SE</i> .....	747.7	596	.000	6.68				
2. <i>COE CSO SE</i> .....	646.5	512	.000	6.18	1–2	101.2	84	.097
3. <i>COE CSO CSE</i> .....	524.4	456	.015	5.42	2–3	122.1	56	.000
4. <i>COE CSO CSE SOE</i> .....	347.7	336	.319	4.14	3–4	176.7	120	.001

of fit of the various models using the likelihood ratio test (measured by  $G^2$ ), and we take  $P = .05$  as our significance level. The table also shows the index of dissimilarity ( $\Delta$ ) for each model and likelihood ratio tests comparing the goodness of fit of selected pairs of models.

Model 1 fits the data in Italy and Poland and model 2 in Ireland: these are the preferred models in these cases. In Germany, Great Britain, and Sweden, model 3 fits the data, but only in Sweden is this the preferred model (because model 4 does not improve on it). In Germany and Great Britain, model 4 fits the data and is our preferred model because it is a statistically significant improvement on model 3, whereas in France and the Netherlands, model 4 is the only one that fits the data. In Hungary the situation is, from our point of view, even worse, because none of the models fits the data. Here the association between class origins and educational attainment varies across surveys in different ways in different cohorts. But the German, French, British, and Dutch cases are also somewhat problematic, because the preferred model for these countries is one in which the marginal distributions of both class origin and education, and also the origin-education association, are not constant over surveys within cohorts. However, because our aim is to model the association between class origins and educational attainment in each cohort, what we really need to know is whether, within each cohort taken separately, this association varies over surveys. We can test this straightforwardly by fitting the model of common (across surveys) association between origins and education to the observations of each cohort.<sup>10</sup> Table 4 shows the results of fitting this model to each cohort in all five of our problematic cases. In Great Britain and the Netherlands there is no evidence for significant change across surveys in the origin-education association in any cohort (the model of common association fits the data), whereas there is some change in the youngest cohort in Germany and in the oldest cohort in France. But in Hungary the association changes in every cohort—a

<sup>10</sup> Though, obviously, we can do this only in respect of cohorts that have been observed twice or more.

TABLE 4  
GOODNESS OF FIT OF MODEL OF CONSTANT (across Surveys) ORIGIN-EDUCATION  
ASSOCIATION WITHIN BIRTH COHORTS

COHORT	GERMANY		FRANCE		GREAT BRITAIN		HUNGARY		NETHER- LANDS	
	$G^2$	$df$	$G^2$	$df$	$G^2$	$df$	$G^2$	$df$	$G^2$	$df$
1908–24 ...	58.5	48	69.6*	48*	29.1	20	60.9*	36*	88.0	72
1925–34 ...	71.6	72	85.0	72	45.7	40	56.9*	36*	126.9	120
1935–44 ...	131.4	120	68.2	72	92.8	80	81.2*	54*	130.4	120
1945–54 ...	99.8	96	58.2	48	64.3	60	66.5*	36*	117.3	96
1955–64 ...	94.0*	72*	23.6	24	14.7	20	40.6*	18*	61.7	48

\*  $P < .05$ .

not unexpected result given that none of the models reported in table 3 fits the Hungarian data.

Our assessment of the data shows that only in Italy and Poland are the marginal distributions of class origins and education constant across surveys for samples from the same cohorts. In Ireland the educational distribution remains unchanged but the class origin distribution does not. In the other countries both marginal distributions change, but, with the exception of Hungary, we could not discern any change in the association between origins and education across surveys within each cohort.<sup>11</sup> The latter is the crucial test. Hungary's failure to pass this test led us to exclude it from the substantive analyses of the article.<sup>12</sup>

In the remainder of our analyses we nevertheless always control for survey effects, that is, the impact of survey period on the relationship between class origins and educational attainment. Because cohorts and periods are correlated in our data—cohorts are not randomly distributed over surveys—failing to control for survey effects may lead us to overestimate cohort change. Our interpretation of such survey effects is that

<sup>11</sup> With the single exceptions for Germany and France as noted above.

<sup>12</sup> Why do the Hungarian data fail this test? In some respects it is quite surprising given that all four Hungarian data sets were collected by the Central Statistical Office and should be quite comparable. One possible explanation that has been suggested to us is the prevalence of extensive adult education in Hungary and a practice of assigning educational qualifications according to the job held (a phenomenon that occurred particularly during the upheavals of the early years of communism). If, however, we analyze each of the Hungarian data sets separately (these results are not shown but are available on request from the authors), we find that educational inequalities decline across birth cohorts in the 1972 survey (in which we observe the three oldest cohorts), the 1983 survey (in which we observe all except the youngest cohort), and the 1992 surveys (in which we observe all cohorts), but not in the 2000 survey (in which we observe only the three youngest birth cohorts). This pattern is very similar to that observed for the other eight countries.

they arise because the conditions outlined earlier (n. 8) are not met. On the one hand, the association between class origin and education in a particular cohort in the population may change over time, mainly because of migration.<sup>13</sup> On the other hand, the relationship between class and education in a sample of data from a cohort may change, even if the relationship in the population remains the same, because of differences in how the sample is drawn from the population, in the response rates of the sample, and in various sources of survey-related error. Further analyses not reported here, as well as some of our later analyses, show that these survey effects are themselves largely random in their impact on the class origin–educational attainment relationship. In other words, it transpires that our results, and our conclusions about change over cohorts, are insensitive to whether we include survey effects or not.

#### Modeling the Data

We noted earlier that the educational categories we use do not have a single sequential ordering, and so the Mare model of educational transitions would not be appropriate. Furthermore, because we lack data on the educational pathways that individuals followed, we could not apply the extended educational transition model of Breen and Jonsson (2000).<sup>14</sup> We focus, therefore, on modeling the joint distribution of class origins and highest level of education using the ordered logit model.

Letting  $Y$  be the random variable measuring educational attainment, we can write the probability that  $Y$  is less than a particular level of education,  $j$ , given background variables  $X$ , as  $\gamma_j(x)$ . There exists a family of statistical models that sets  $g[\gamma_j(x)] = t_j - \beta'x$  (McCullagh 1980), where  $g$  is a link function (such as the logit, the inverse normal, or the complementary log-log) that maps the  $(0, 1)$  interval into  $(-\infty, \infty)$ . The logit link, which yields the ordered logit model, sets  $g[\gamma_j(x)] = \ln[\gamma_j(x)]/[1 - \gamma_j(x)]$ , and for any two observations with covariate values  $x$  and  $x'$  we get

$$g[\gamma(x)] - g[\gamma(x')] = \ln \frac{\gamma(x)}{1 - \gamma(x)} - \ln \frac{\gamma(x')}{1 - \gamma(x')} = \beta(x' - x),$$

and so the log odds ratio of exceeding, or failing to exceed, any particular threshold is independent of the thresholds themselves and depends only on the betas.

<sup>13</sup> It may also change if people really do acquire more education and differ according to their class origins in their likelihood of doing so, but given our lower age limit of 30 years, the impact of this on our data is likely to be minor.

<sup>14</sup> Strictly speaking, the same limitations apply to the data used in most of the Shavit and Blossfeld country studies.

The ordered logit model has two attractive properties: it is more parsimonious than the multinomial logit or the Mare model, and, provided that the ordered logit is the correct model for the data, the estimates of the  $\beta$  parameters are unaffected by collapsing adjacent categories of  $Y$ . This invariance means that if the educational system makes finer distinctions than have been captured in the variable  $Y$ , the  $\hat{\beta}$ 's are unaffected by our having measured education less precisely.<sup>15</sup> It also implies that, when making comparisons, provided that the same stochastic ordering of categories is used in all samples, we do not require the categories to be defined in exactly the same way. This facilitates both temporal and cross-national comparisons. This property is not shared by the educational transition model or by multinomial logit models, whose estimates are tied to particular categorizations of educational attainment. But the advantages of the ordered logit come at a price: the model assumes that class inequalities are identical at each level of education.<sup>16</sup> This is sometimes called the parallel slopes or proportional odds assumption and is frequently found not to hold. We address this problem in our analyses.

In table 5 we report the goodness of fit of the ordered logit models in which we control for survey (cols. 1–3) and in which we do not (cols. 4–6). In all models the thresholds,  $t_j$ , are allowed to vary over cohort: in the models in columns 1 and 4, class origin effects are allowed to vary over cohorts, whereas in columns 2 and 5 they are held constant. The final column in each case shows the deviance comparing the model of change against the model of constancy in class effects. This shows significant change everywhere except Italy (although the Irish data barely reach significance when we control for survey period). The deviance associated with this change is lesser when we control for survey effects compared with when we do not, so our conclusions about change are unaffected, though the magnitude of that change is a little smaller.

One can also see, however, that, with the .05 level of significance, the ordered logit model fails to fit the data in almost all countries, suggesting that the parallel slopes assumption does not hold. Later in the article we discuss the departures from proportionality in our data and we find that they are not consequential, so for the most part we base our findings on

<sup>15</sup> Han and Hausman (1990, p. 2) point out that the efficiency of the estimates will depend on the fineness of the categorization.

<sup>16</sup> To be more specific, the log odds ratio, comparing students of different class origins, of exceeding, rather than failing to exceed, a given level of education is the same at all levels of education. This does not imply that the log odds ratios at all educational transitions (as these are defined in the Mare model, e.g.) are the same, as we show later (except in the special case of the cumulative probability model with the complementary log-log link).

TABLE 5  
 MODEL FITS FOR ORDERED LOGIT MODELS, BY COUNTRY, SURVEY, AND COHORT:  
 CHI-SQUARED SUMMARY MEASURE OF CHANGE OF *OE* EFFECTS ACROSS COHORTS  
 FOR *SCOE* DATA TABLE (Controlling for *SOE*) AND *COE* DATA TABLE

COUNTRY	DATA FROM <i>SCOE</i> TABLE, CONTROLLING FOR <i>SOE</i>			DATA FROM <i>COE</i> TABLE (Collapsed over <i>S</i> )		
	Change <i>COE</i>	No <i>OE</i> Change	$\Delta G^2$	Change <i>COE</i>	No <i>OE</i> Change	$\Delta G^2$
	(1)	(2)	(3)	(4)	(5)	(6)
Germany .....	557.0 (468)	642.0 (492)	84.9 (24)	134.4 (90)	234.6 (114)	100.2 (24)
France .....	515.0 (312)	629.3 (336)	114.3 (24)	283.5 (90)	459.5 (114)	176 (24)
Italy .....	90.1* (96)	111.0* (114)	20.8* (18)	43.7* (48)	67* (66)	23.3* (18)
Ireland .....	189.2 (130)	221.1 (150)	31.8 (20)	101.8 (50)	155.3 (70)	53.4 (20)
Great Britain ...	709.9 (275)	780.0 (295)	70.1 (20)	502.7 (75)	604.4 (95)	101.7 (20)
Sweden .....	624.1 (450)	723.4 (474)	99.3 (24)	286.2 (90)	413.2 (114)	126.9 (24)
Poland .....	191.5 (115)	318.8 (135)	127.3 (20)	192.5 (50)	351.3 (70)	158.8 (20)
Netherlands ....	613.5 (516)	694.3 (540)	80.8 (24)	154.1 (90)	266.5 (114)	112.4 (24)

NOTE.—Numbers in parentheses are degrees of freedom.

\* Not significant at  $P < .05$ .

the results of the models whose goodness of fit is reported in column 1 of table 5 and whose parameter estimates are shown in figure 4.

In figure 4 each line refers to a class origin and shows how the coefficients for that class evolve over cohorts, with class I always acting as the reference category (classes I+II in Ireland and Poland and I+II+IVa in Britain) and having a coefficient of zero. The overall impression is of a decline in class inequalities everywhere (even in Italy, where the change is not statistically significant), but figure 4 also allows us to see how the relative positions of the various classes have shifted. In Germany there was a general narrowing of class differentials in educational attainment following the 1925–35 birth cohort and continuing until the 1945–54 cohort. Little change is evident between the latter and the youngest cohort. In France equalization started later, with the 1935–44 cohort, and continued longer, persisting into the 1955–64 cohort. It was slightly more differentiated by class origins than in Germany because men from class III showed no change in their position relative to men from class I, whereas men of farm origins (class IVc) experienced a particularly marked im-

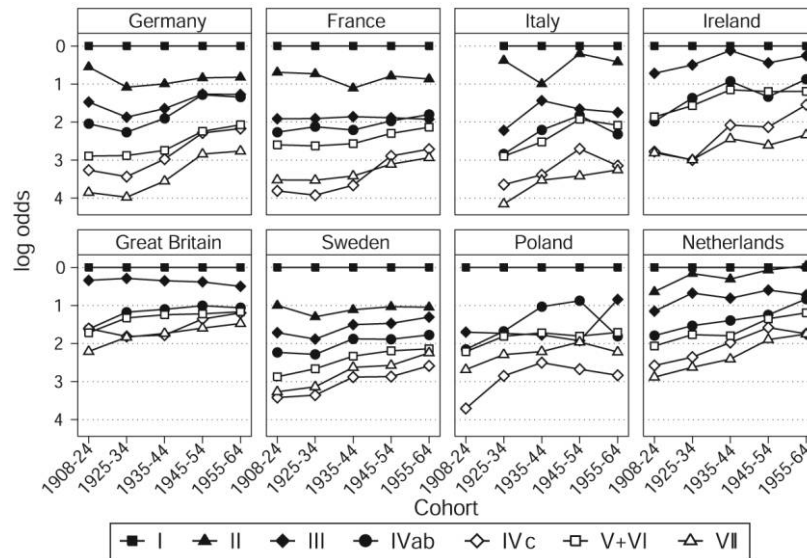


FIG. 4.—Ordered logit models for educational attainment in eight countries for men. Class origin effects over cohorts, controlled for survey effects. Class I is classes I+II in Ireland and Poland and classes I+II+IVa in Britain; educational levels 3a and 3b are merged in Italy and Ireland.

provement in their relative position. In Sweden all classes have improved their position relative to class I, as in Germany, and the decline in inequality largely took place throughout the whole observation period. The Dutch case too shows a general tendency toward a narrowing of class differentials; this occurs over all cohorts, for the most part, and classes IVab, IVc, V+VI, and VII have made the greatest gains. In contrast with these four countries, the British picture is more complicated. Although all classes except III are less differentiated from class I in the youngest cohort than they were in the oldest, almost all the improvement for classes IVb and V+VI occurred between the 1908–24 and 1925–34 cohorts, whereas for class VII there has been a more prolonged improvement and for class IVc an improvement from the 1935–44 cohort onward. In Poland there has been a steady improvement in the relative positions of classes IVc, V+VI, and VII until the middle cohort, at which point the position of IVc worsens, followed by a worsening of the positions of IVab and VII in the youngest cohort. In Italy we find a modest decrease in class inequalities until the 1945–54 cohort, after which class inequalities tended to reassert themselves. Class VII is an exception because its position continues to improve, albeit slightly. Finally, in the Irish case, over the five cohorts the distance between class I and all other classes was reduced,

and the positions of the petty bourgeoisie (IVab) and farmers (IVc) have improved noticeably.

Overall it seems to be the most disadvantaged classes that experienced the greatest improvement in their position over the first two-thirds of the 20th century. Whereas the position of class II, relative to that of class I, tends to show little change and classes III and IVab show improvements in some countries and in some cohorts but not in others, classes IVc, V+VI, and, particularly, VII show general and widespread improvement in their position vis-à-vis the other classes. As to the more precise timing of the change, we can note that we observe a trend of declining inequality in all countries for the cohorts born between 1935 and 1954, in particular for the least advantaged classes. These are the cohorts that made the crucial educational transitions in the first three post-World War II decades up to about 1975, during which there were significant improvements in living conditions during a period of strong economic growth in most European countries.

To check the implications of the proportional odds assumption, we have fitted a model that relaxes the assumption where this is necessary to achieve a good fit to the data.<sup>17</sup> This model is the generalized ordered logit, characterized by the inclusion of threshold-specific class origin coefficients,  $\beta_j$ , for some classes. In one way the generalized ordered logit can be understood as a model that lies somewhere between the ordered and the multinomial logit. It relaxes the parallel slopes assumption by allowing particular social class origins to have different  $\beta$  values at different thresholds rather than a common parameter across all thresholds. If we were to let this hold true for all classes, we would arrive at a model that would be equivalent to the multinomial logit. The *gologit2* procedure allows threshold-specific  $\beta$ 's only where they are needed to make the model fit the data at the .05 level. It operates inductively, and we use it simply as a means by which to check the robustness of our substantive conclusions to departures from the parallel slopes assumption.

In figure 5, which reports results for France, these coefficient estimates are shown as the small symbols centered around the line for each class, where the line is that estimated for the model shown in figure 4. For each cohort and class, the four symbols from left to right indicate the class origin parameters for successively higher levels of education. We focus our discussion on France because it constitutes the most complex case. Figures for the other countries are given in appendix figure A1.

The reading of the figure for origin class II in France can be illustrated as follows: in the oldest cohort the probability of exceeding the first ed-

<sup>17</sup> We estimated these models in Stata with *gologit2* (Williams 2006). We would like to thank Richard Williams for providing this ado-file and his helpful support.

## Educational Attainment

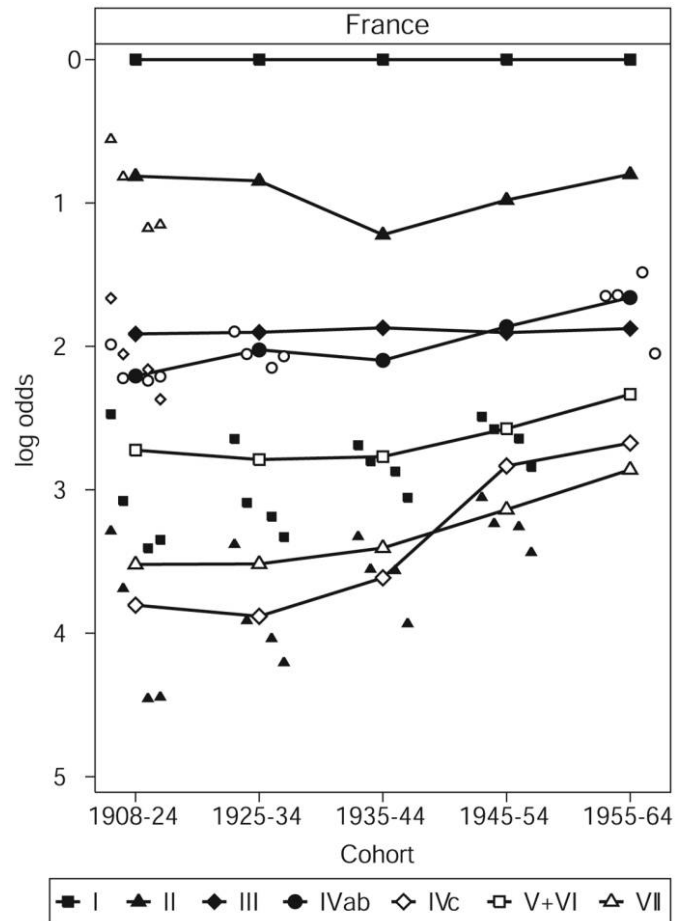


FIG. 5.—Ordered logit models for educational attainment in France for men; violations of proportional odds assumption displayed.

educational level among class II deviates slightly less from class I than in the model assuming proportional odds (the symbol is above the line), whereas the probabilities of exceeding educational level 3 (upper secondary) or 4 (lower tertiary) deviate more (the third and fourth symbols are below the line). For men in class II in all other cohorts there are no significant deviations from the proportional odds assumption. For all other classes (except farmers, for whom the proportionality assumption holds for all cohorts) we see two consistent regularities in France: first, as for the oldest cohort of class II, inequalities in the chances of exceeding a given level of education tend to increase as we move from the lowest to



the highest educational level. Second, the deviations from proportional odds are largest for the oldest cohort and tend to become successively smaller for younger cohorts. With the exception of class IVab, no significant deviations are found for the youngest cohort. For the other countries (see app. fig. A1) deviations from proportionality are fewer and smaller than in France, and in their pattern they tend to mirror the French case. In examining these deviations from the proportional odds assumption, our interest lies in whether they are systematic in ways that would lead us to alter our conclusions about patterns of change or that would give us further details about this pattern. The lines from the simple ordered logit model represent weighted averages of the threshold-specific coefficients. They give us, in some cases, an overestimate of the decline in class inequality in the odds of exceeding lower educational levels but a conservative estimate of the decline in class inequality in the odds of reaching the higher educational levels. All this is clearest in the French case, but, taken over all eight countries, the extent of under- or overestimation is either small or, where the ordered logit fits the data, nonexistent.

#### METHODOLOGICAL VARIATION

In this section of the article we tackle the question of whether, and if so how, our results about temporal change might differ had we made different decisions about how to investigate the question of educational inequality. We explore four broad areas: first, we compare the results of an ordered logit analysis with those using the Mare model; second, we compare the results from our model using class origins with those from a model in which, as in Shavit and Blossfeld, parental education is also included as an indicator of social origins; third, we ask whether differences in the coverage of cohorts and periods might explain the different findings; and, finally, we ask how likely we would have been to find significant change had we analyzed samples equal in size to those used by the authors of the country chapters in the Shavit and Blossfeld volume.

##### The Mare Model and the Ordered Logit

As we pointed out earlier, we have chosen the ordered logit rather than the Mare model because the educational categories that we use are not ones that students pass through sequentially and, furthermore, we have no information on which pathways our respondents followed in their educational career. Nevertheless, because the Mare model was used in all the analyses in the Shavit and Blossfeld volume, the question of whether we would have gotten different results had we used it cannot be avoided.

And so we address the question as follows. We define four educational levels: 1abc, 2ab, 2c, and 3ab. They do not form a strict sequence, since in some educational systems students progress directly from 2ab to 3ab, but they come reasonably close to being sequential. These then define three transitions, from primary education, 1abc, to 2ab or above; from intermediate secondary, 2ab, to 2c or above; and from upper secondary, 2c, to tertiary, 3ab. We fit the Mare model to these data, and for comparison purposes we also fit the ordered logit model using these four educational categories.<sup>18</sup> And third, we compute the transition odds ratios (i.e., the same thing as the Mare model estimates) using the ordered logit results, following the formula given by Breen (2007). We do this only for Germany and France, for the purpose of making a comparison between the actual Mare model estimates and these implied estimates. These three steps allow us to determine whether the Mare model and the ordered logit yield different conclusions when applied to the same data: in other words, are the different findings due to the choice of a different statistical model?

Figure 6 shows the results from the Mare models and the ordered logit model fitted to the same data. For each of the eight countries we show the class-specific parameters per cohort for the transitions from primary education, from intermediate secondary, and from upper-secondary to tertiary education, and then we show the ordered logit coefficients. It is immediately evident that, as expected, the ordered logit results very closely replicate those shown in figure 4. But it is equally clear that, for some countries, the Mare model reveals a marked increase in equality in the transition rates out of primary education. In fact, we find statistically significant weakening of class inequalities at that transition in Germany, France, Britain, Sweden, Poland, and the Netherlands. For Italy and Ireland the pattern is very unclear, and this is probably once again a consequence of the rather small samples in these countries. These results are quite different from those reported by Shavit and Blossfeld, who argue that there was no change across birth cohorts at any educational transition. Their country chapters confirm this: even in the transition from primary to secondary education, there appears to be no decline in social origin inequalities. “There are only two exceptions to this pattern: Sweden and the Netherlands in which the effects of father’s occupation and education on the low and intermediate transitions declined” (Blossfeld and Shavit 1993, p. 21). We also see that, in the main, inequalities at later transitions are smaller but show little in the way of a trend toward lesser or greater class differences. In France, for example, the figure suggests

<sup>18</sup> But the results we obtain should be almost identical to those reported in fig. 4 because the ordered logit estimates are robust to collapsing of adjacent categories in the dependent variable (in this case, 3a and 3b).

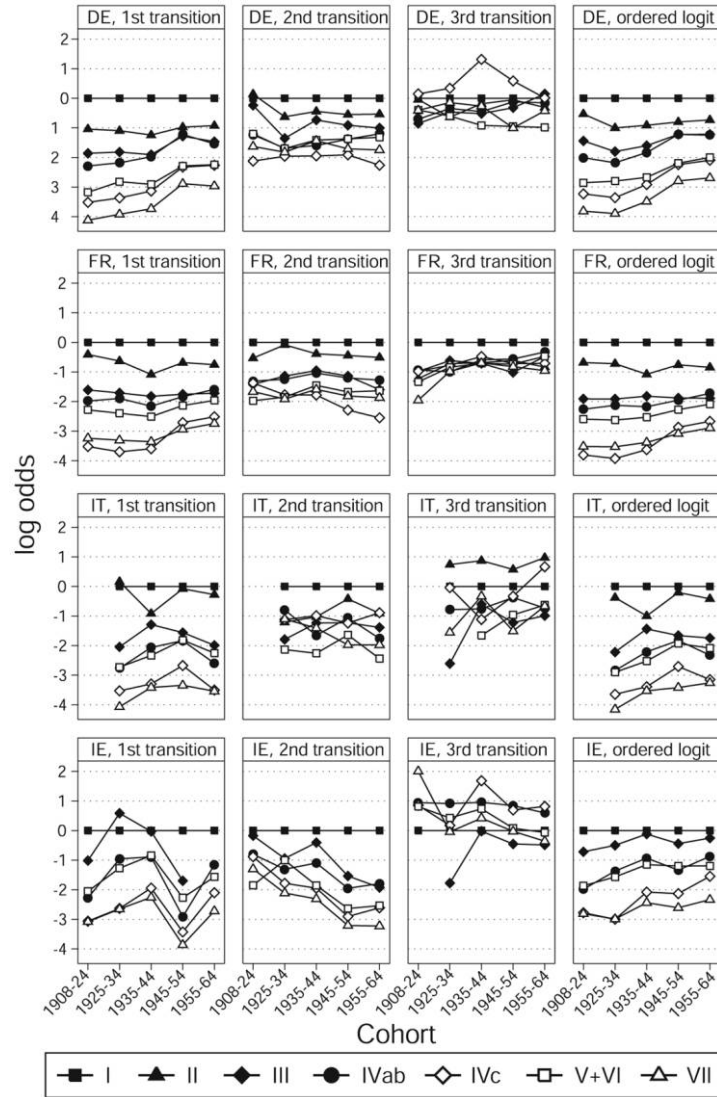
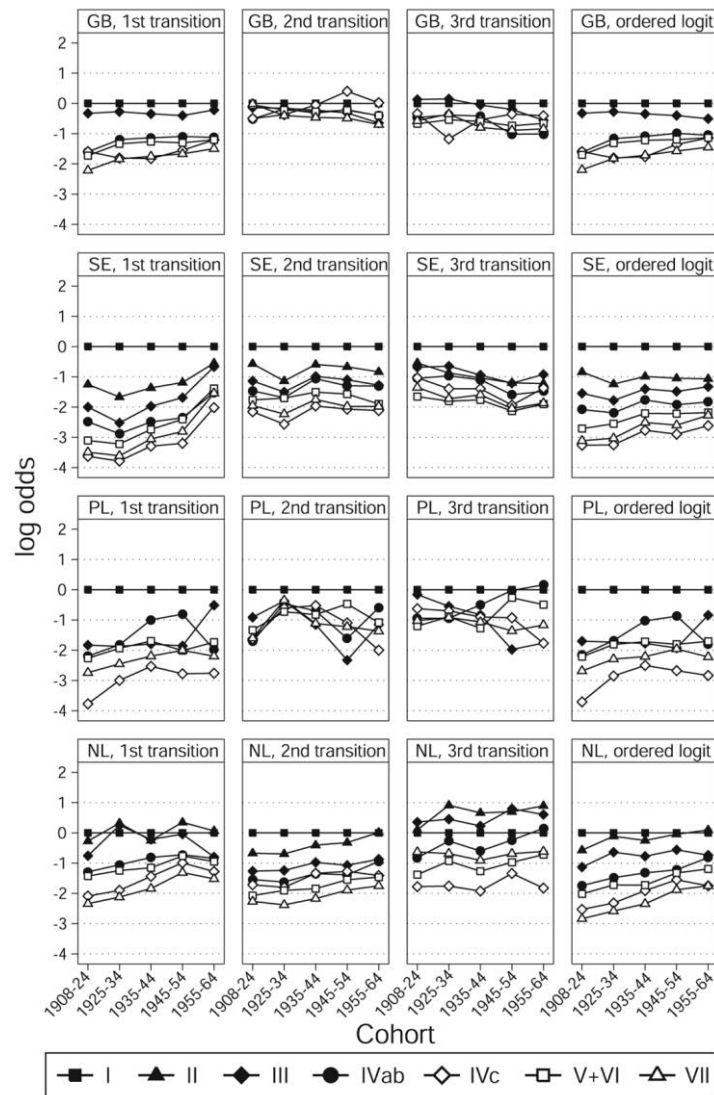


FIG. 6.—Binary logistic regressions on successive educational transitions (Mare model) and ordered logit models for educational attainment in eight countries for men. Class origin effects over cohorts, controlled for survey effects. All ordered logit models merge educational



categories 3a and 3b; class I is classes I+II in Poland and classes I+II+IVa in Britain. For graphical reasons, the following coefficients are not displayed: Italy, class VI, cohort 2, transition 3; Ireland, class III, cohort 1, transition 3; Ireland, class III, cohort 5, transition 1.

some decline in inequality at the transition to tertiary education, whereas it suggests an increase in inequality at the second transition in Ireland. But, in fact, we find no significant change in any country in inequalities in the transition to tertiary education, and in only two cases—France and Sweden—do we find significant differences in the transition from intermediate secondary education. In neither France nor Sweden, however, is there evidence of a trend.<sup>19</sup>

The Mare model and the ordered logit measure inequality differently: the Mare model is concerned with inequality in the log odds of making a given transition conditional on being at risk of doing so, whereas the ordered logit provides a measure of class inequalities in the odds of exceeding any particular educational level. For this reason it is difficult to compare the results of the two models as they are shown in figure 6, but comparison is made easier by the fact that the ordered logit parameters can be used to derive estimates of educational transition odds ratios (and so the ordered logit yields estimates of two important quantities: the unconditional odds ratio of reaching a particular educational level and the conditional odds ratio for the transition from one level to the next). For a given social class, with coefficient  $\beta$  in an ordered logit model, the transition odds ratio for the  $j$ th transition, with class I taken as the comparison, is given by the formula (Breen 2007)

$$\log \frac{1 + \exp(t_{j-1} - \beta)}{[1 + \exp(t_{j-1})] \exp(-\beta)}.$$

An important feature of the expression for the educational transition odds ratios implied by the ordered logit is that they are a function of both the  $\beta$  parameters and the thresholds.

Figure 7 shows these quantities computed for two countries, Germany and France. Comparing them with the relevant parts of figure 6 shows that they are similar to, though not identical with, the Mare model estimates.<sup>20</sup> The ordered logit implies a trend toward greater equality in odds ratios at the first transition that is very similar to that shown by the Mare model. Likewise, in the German case, the implied odds ratios for

<sup>19</sup> In both countries the odds ratios increase in the 1925–34 cohort but then return to their 1908–24 level.

<sup>20</sup> It is noticeable that the lines for the last transition are smoother in fig. 7 than in fig. 6. The reason is that in the former, all the observations are used to estimate the transition odds ratios, whereas in the latter, only those who reached educational level 2c are included in the model. One consequence of this is that, if the ordered logit is the correct model for the data and if the Mare model is subject to selection bias from unmeasured factors influencing whether or not students continue to the next level of education, then the ordered logit transition odds ratios are unbiased estimates of the true transition odds ratios and the Mare model parameters are not.

## Educational Attainment

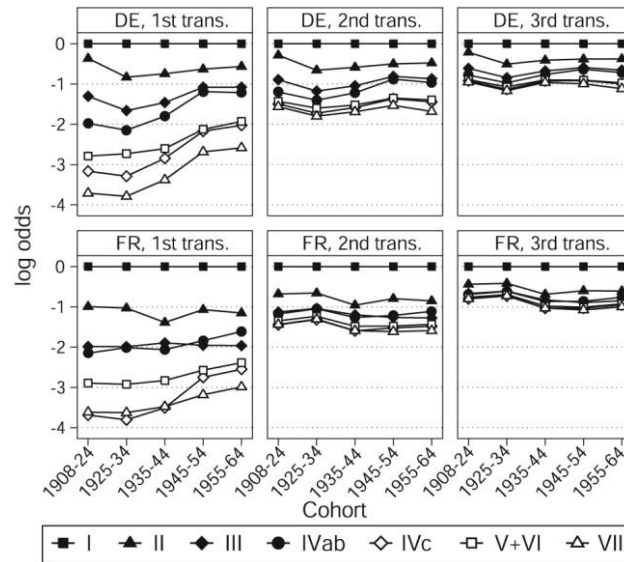


FIG. 7.—Implied Mare models from the ordered logit model for men in Germany and France. Transitions: 1abc versus 2abc3ab; 2ab versus 2c3ab; 2c versus 3ab.

the second and third transitions show no trend, as was the case for the Mare model. For France, the figure suggests some growth in inequality in both the second and third transitions that occurred between the cohorts born 1925–34 and 1935–44; this was not evident in the Mare model. But this occurs in the context of an overall decline in class inequalities in the odds of reaching tertiary education. Over successive birth cohorts, greater equality at the transition from primary education has meant that, notwithstanding a slight increase in inequality at the transitions to the upper-secondary level and from upper-secondary to tertiary education, the overall, unconditional odds of a lower-class student's reaching tertiary education have increased more than the same odds for students from higher-class backgrounds.

## Parental Education

Does the inclusion of a measure of parental education influence our findings? We can address this question in respect to six out of our eight countries because we have no information on parental education for respondents in Britain and Poland. Furthermore, for Germany and Sweden, we lose about half of the observations because not all surveys in these

countries collected data on parental education.<sup>21</sup> For all six remaining countries, we use a measure of father's education because we lack data on mother's education for almost all countries, particularly in early surveys. Father's education is coded into four educational categories: primary education (1abc), lower-secondary education (2ab), upper-secondary education (2c), and tertiary education (3ab).

Figure 8 shows the class effects on educational attainment, controlling for father's education. For all countries, controlling for father's education leads to some reduction in the size of class effects, because parental education is correlated with parental class. However, for all countries for which we earlier found reductions in class effects across cohorts, the decline is also evident when we control for father's education.

Figure 9 shows the effects of father's education, controlling for social class. For all countries except Ireland, these effects decline across cohorts, though this is clearly much less pronounced than the decline in class origin effects. Thus for most of our countries, both effects of social origin change in the same direction, and it is not the case that the reduction in class effects is counterbalanced by stable or even increasing education effects.

As well as including education, most of the chapters in *Persistent Inequality* use father's job prestige as their indicator of social origin, but our data do not include any measures of parental socioeconomic status or prestige. However, Müller and Haun (1994) reanalyzed a subset of the German data used in the present study with prestige as the measure of parental occupation. They also found less indication of decline over time with this indicator. It is also worth noting that the three analyses in *Persistent Inequality* that measure social origins using social class find the same results that we do (changes in Sweden and the Netherlands and no change in Italy).

#### Coverage of Periods and Cohorts

Might a difference in the cohorts included in the data have led to different results? For the most part our data refer to cohorts born between 1908 and 1964. In the Shavit and Blossfeld volume the choice of birth cohorts is remarkably similar. By country they are as follows: Germany, 1916–65 (and, as we noted earlier, our German cohorts are born 1915–64); Italy, 1920–61 (and, in our analysis, we use Italian cohorts born 1925–64); Great Britain, 1913–52; Sweden, 1902–67; Poland, 1920–69; and the Nether-

<sup>21</sup> For the Netherlands, we lose about 15% of our cases; for France, Italy, and Ireland, less than 3%. Missing information on parental education occurs in an unsystematic way. The restricted sample with valid data on parental education shows the same trend of declining class effects over cohorts as the full sample.

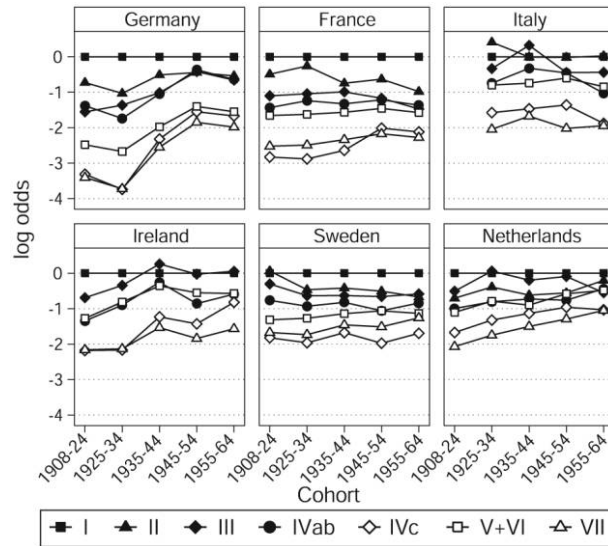


FIG. 8.—Ordered logit models for educational attainment. Class origin effects over cohorts, controlled for father's education and survey effects. Class I is classes I+II in Ireland; educational levels 3a and 3b are merged in Italy and Ireland.

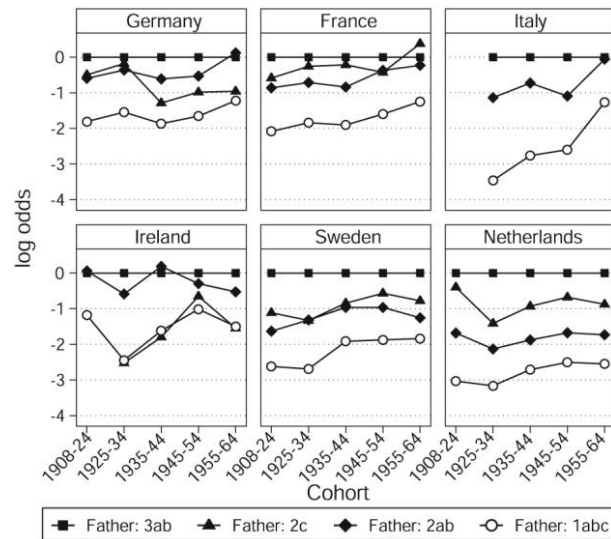


FIG. 9.—Ordered logit models for educational attainment. Effects of father's education over cohorts, controlled for class origin and survey effects. Educational levels 3a and 3b are merged in Italy and Ireland; father's educational level 2c is missing in the first cohort in Ireland and for all cohorts in Italy.



lands, 1891–1960.<sup>22</sup> The two main differences are found for the Netherlands and Great Britain. The Dutch data in the Shavit and Blossfeld volume have older cohorts than we do; nevertheless, our conclusions concerning educational inequality are the same. In the British case we have younger cohorts than in the Shavit and Blossfeld volume: Could this account for the difference in our results? This seems unlikely because the extra cohort in our data (born 1955–64) shows little change in the extent of educational inequality from the previous cohort. And, indeed, if we omit the youngest cohort from our analysis, we still find significant change in educational inequality (the deviance associated with this change is 59.6 with 16 *df*, including survey period effects, and 78.2 with 16 *df*, excluding survey period effects) and the same trend.

Another potentially important difference is that, whereas we apply a lower age limit of 30 years, the country chapters in the Shavit and Blossfeld volume do not always do so. One consequence of this is that, in some countries, the youngest cohorts are observed at an earlier age. So, for example, the data used in the German chapter (Blossfeld 1993) were collected between 1984 and 1988, which means that persons in the youngest cohort (born between 1961 and 1965) would have been between 19 and 27 years old. Given the structure of the German educational system, many of them would not have completed their education. But in this case, the youngest cohort was not included in the analysis of those later educational transitions, which its members might not have yet reached. And this practice was also followed in the Swedish chapter (Jonsson 1993).

In the analysis for the Netherlands, de Graaf and Ganzeboom (1993) impose a lower age limit of 25, and their youngest cohort is made up of those 25–36 years old. In those countries that use only a single survey, it is possible to calculate the ages of the persons included in the youngest cohort: 20–29 in Great Britain, 24–37 in Italy, and 18–27 in Poland. In all four cases the distribution of educational attainment will have been truncated in the youngest cohort. However, this truncation will affect only transitions to tertiary education, because in all of these countries, all earlier transitions will have been completed before the lower age limits, with the possible exception of the very youngest respondents in the Polish data. In other words, those country studies in Shavit and Blossfeld that did not exclude the youngest respondents from the analysis of the transition to tertiary education will have obtained biased estimates of social origin inequalities at this transition. But this cannot explain why their results

<sup>22</sup> Blossfeld and Shavit (1993, p. 13, table 1.1) report the Italian cohorts as covering 1920–67 and the Polish, 1912–69. Cobalti and Schizzerotto (1993, p. 159) exclude from the Italian data individuals born after 1961, and Heyns and Bialecki (1993, p. 304) report that their Polish cohorts were born between 1920 and 1969.

and ours differ because our finding of change in inequality is driven by equalization at the earlier educational transitions, not at later ones, and inequality in the earlier transitions is, to the best of our knowledge, estimated adequately in the Shavit and Blossfeld country studies despite the truncated data for the youngest cohorts.

### Sample Size

A more likely source of the discrepancy between our results and those of Shavit and Blossfeld is a difference in sample size: several of the country studies in *Persistent Inequality* are based on rather small samples and may have lacked the statistical power to find change over time. Random fluctuation in the small samples that were available for each cohort may have been larger than the systematic change that occurred from cohort to cohort. To check the accuracy of this argument we drew 100 random subsamples from our data for each country included in our analysis and in *Persistent Inequality* (Germany, Italy, Britain, Sweden, Poland, and the Netherlands) corresponding in size to the samples used for the analyses in the latter. To each of these subsamples we fitted the uniform difference (unidiff) or log-multiplicative layer effect model (Erikson and Goldthorpe 1992; Xie 1992) of change over cohorts. In this model the origin-education association varies over cohorts log-multiplicatively according to a single parameter for each cohort (for details, see Xie [1992]), in this case normalized by setting its value for the oldest cohort to 1. The model says that the difference between two cohorts in any log odds ratios involving origins and education is proportional to the difference in their values of this parameter: declining values over cohorts thus correspond to a weakening in the association between origins and education.<sup>23</sup> In figure 10 we plot the cohort-specific parameters from each of our 100 subsamples.

In the two cases in which *Persistent Inequality* reports a weakening of the origin-education association, namely Sweden and the Netherlands, figure 10 shows that from our data it would be difficult to draw a sample of the size used there that did not show such a weakening. Conversely, in the other four countries, from subsample to subsample there is a high degree of instability in the pattern of results, and it would therefore be quite possible to draw a sample that showed no significant change even in those three cases (Germany, Britain, and Poland) in which we know that such a change occurred.

<sup>23</sup> This is a very parsimonious model that captures change in a single parameter, but its parsimony comes at the price of the restrictive assumption that proportionality between the log odds ratios within the origin-education table is constant over all cohorts.

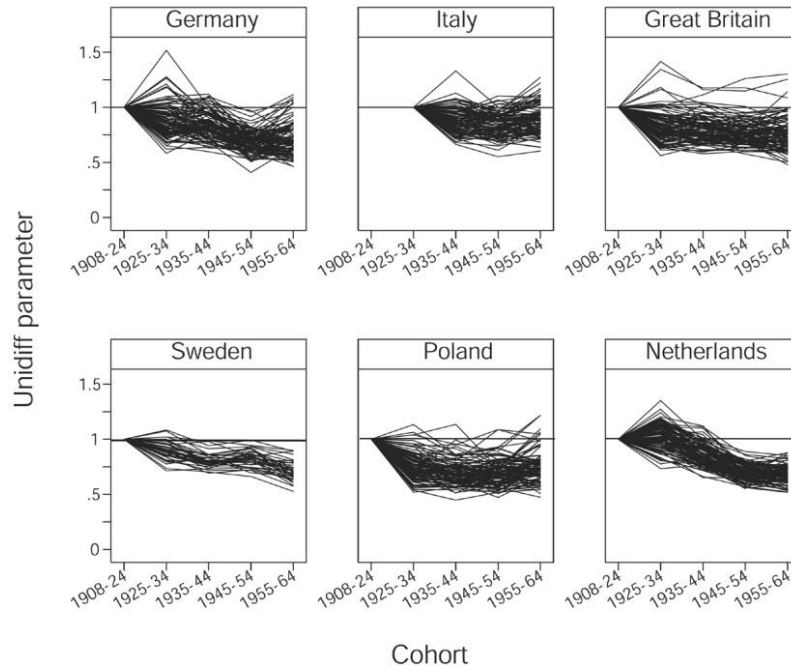


FIG. 10.—Log-multiplicative evolution over cohorts of the origin-education association by country; 100 samples drawn from the data used in this article of the size of the samples analyzed in Shavit and Blossfeld (1993).

These comparisons are made more formally in table 6, which shows the results of pairwise comparisons across cohorts using the unidiff model as before. In the first case we compared the oldest and youngest cohorts, and in the second we compared the second oldest with the youngest. We made these comparisons using two different data sets: our own full data and samples from our data equal in size to two-fifths of that reported in the relevant country chapters of Shavit and Blossfeld (because we are comparing two out of five birth cohorts).<sup>24</sup> For the latter we ran 1,000 simulations (each based on a different sample from our data) and counted the number in which the unidiff model significantly improved in fit compared with a model of no change.

Columns 1 and 2 of table 6 report the difference in  $G^2$  between the unidiff and constant association models using the full data sets for each

<sup>24</sup> That is, we drew samples from each birth cohort in our data equal in size to one-fifth of the samples used by the authors of the chapters in Shavit and Blossfeld's volume. This means that the two cohorts we compare have equal sizes in our simulations and that the total sample size is two-fifths of that in Shavit and Blossfeld.

TABLE 6  
PAIRWISE COMPARISONS USING THE UNIDIFF MODEL

COUNTRY	$G^2$ WITH 1 $df$		% RETURNING SIGNIFICANT $G^2$ ( $P < .05$ )	
	Cohorts 1 and 5; All Data	Cohorts 2 and 5; All Data	Cohorts 1 and 5; Restricted Sample Size; 1,000 Runs	Cohorts 2 and 5; Restricted Sample Size; 1,000 Runs
	(1)	(2)	(3)	(4)
Germany .....	29.185	14.538	59.9	29.9
Italy .....	. . .	.019	. . .	.9
Great Britain ...	20.089	1.490	63.9	5.2
Sweden .....	43.094	17.730	97.1	61.5
Poland .....	9.641	.192	17.4	.1
Netherlands ....	14.262	23.650	73.3	86.3

of the six countries. These show significant change between the oldest and youngest cohorts (col. 1) in Germany, Britain, Sweden, Poland, and the Netherlands (we lack data for the oldest cohort in Italy) and significant change between the second oldest and youngest cohorts in Germany, Sweden, and the Netherlands (col. 2). Columns 3 and 4 show the percentage of our 1,000 simulations in which we find a significant  $G^2$ . We see that, in column 3, notwithstanding the fact that our data report a significant change between the oldest and youngest cohorts in five countries, we find significant change in only 60% of the simulations for Germany and Britain and 17% for Poland. In other words, there is a good chance, given a sample of the size found in Shavit and Blossfeld's volume, that no change would be detected in these three countries. For the comparison between the second oldest and the youngest cohort, we find (in col. 4) that even though there is significant change in Germany, only 30% of the simulations come to the same finding, and for Sweden, 60% of the simulations detect change. Of course, we are not here drawing samples from the population, so the conclusion we can draw is not as strong as it would be in that case: nevertheless, this exercise strongly suggests that *Persistent Inequality* may have failed to find change in some countries because the samples used there were too small.

#### CROSS-NATIONAL COMPARISONS

Thus far we have concerned ourselves with temporal change, but an important and interesting question is whether different countries show different degrees of class inequality in educational attainment. The difficulties of making cross-national comparisons are well known, but, as

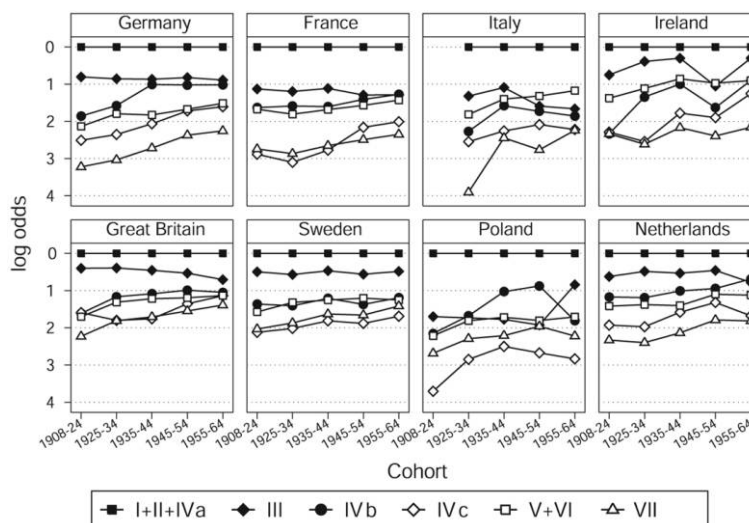


FIG. 11.—Ordered logit models for educational attainment in eight countries; classes and educational degrees standardized across countries. Class origin effects over cohorts, controlled for survey effects. First cohort in Italy dropped because of too few cases; classes IVa and IVb merged in Poland.

noted earlier, the ordered logit model possesses the very useful property of being robust to differences in the precise categorizations of the dependent variable used in the various countries. Provided that the ordering of the categories is the same, comparisons of the  $\beta$  coefficients, and of functions of them, can legitimately be made.

Our first step in making cross-national comparisons was to render the data sets more comparable. Because the British data combine class origins I, II, and IVa, we combined these in all the other countries too; and because the Polish data (which also combine I and II) do not distinguish IIIb from IIIa, we place IIIb in class III in all countries. We also made the educational categories consistent across countries (though this was not strictly necessary). The Italian and Irish data have only four educational categories, and so we reduced the number of educational categories in every country by combining 3a and 3b (lower- and upper-tertiary education). The  $\beta$  coefficients from the ordered logit models estimated on these data are shown in figure 11. As we might have hoped, they are very similar to those of figure 4, but now we are concerned with how the magnitude of the differences between classes compares across the different countries. In the oldest cohort, class differences were large in Germany, France, Italy, and Poland and small in Britain, Sweden, and the Netherlands; by and large, this pattern has persisted over the century. It appears, however,

that the weakening of class inequalities in Germany and France has brought these countries closer to Britain, Sweden, and the Netherlands and has opened a gap between them and Poland. The greater inequality in Poland, however, is wholly due to the very poor position of men from farming origins.

#### ASSESSING THE DEGREE OF CHANGE

The evidence that we have presented thus far in support of our assessment of change in educational inequality has largely relied on graphical displays. But we have also computed two measures that seek to capture such change in a parsimonious and understandable way. Our first measure, based on Hout, Brooks, and Manza's (1995)  $\kappa$  statistic, is simply the variance of the ordered logit coefficients in each cohort. The logic of this is that more variation in the coefficients implies greater class differences, and zero variation implies no class inequalities. Our second measure is the weighted sum of the coefficients in each cohort, with the weights being the proportion of each cohort in the social class to which the coefficient applies. Because all the coefficients for classes II–VII are less than zero, their sum reflects the extent of inequality between classes (which would not be true if they were not bounded by zero). When these coefficients are weighted, however, the impact of a coefficient on the sum will depend on the relative size of the class to which it applies. So this measure could be seen as capturing overall “welfare” insofar as it comprises a sum of disadvantages, each of which is weighted by the proportion of the population that suffers that particular degree of disadvantage.

Figures 12 and 13 show these statistics, derived from the cross-nationally comparable data used to generate figure 11. Thus they are also valid for comparisons between countries. Both measures demonstrate that the decline in class inequality has been large. Between the oldest and youngest cohorts the unweighted variance of the coefficients more than halved in Germany, Britain, Sweden, and Italy and almost halved in France and the Netherlands. They also show quite clearly the temporal pattern of decline, with a more or less linear trend in Germany, France, Britain, Sweden, and the Netherlands tending to flatten out somewhat in the two youngest cohorts. The conclusions to be drawn from figure 13 are similar to those from figure 12 (and also fig. 4) except in the cases of Ireland, Italy, and Poland, where now we see clearer trends toward greater equality. In both the Italian and Irish cases, all the change seems to have occurred between the cohorts born 1925–34 and 1935–44. In both countries an improvement in the position of the most disadvantaged classes—IVc and VII—was reinforced by the rapid decline in their numbers. In

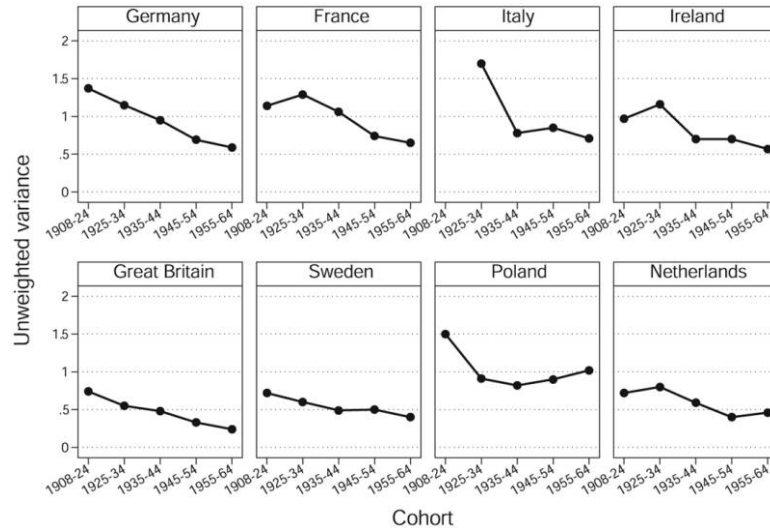


FIG. 12.—The unweighted variance of standardized ordered logit coefficients from fig. 11

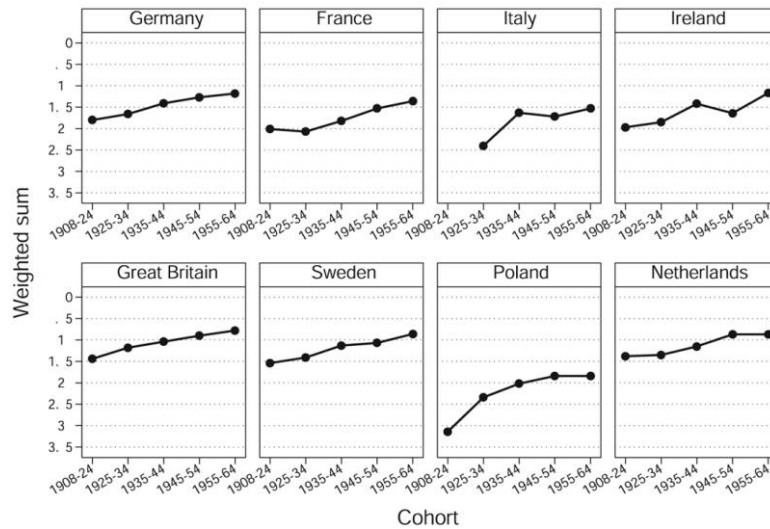


FIG. 13.—The weighted sum of standardized ordered logit coefficients from fig. 11

the Polish case the apparent increase in inequality in the younger cohorts that is evident in figure 12 is replaced by unchanging inequality across these cohorts in figure 13. The explanation for this can be found in figures 3, 4, and 11: in Poland, farmers (class IVc) were a very disadvantaged class whose position initially improved but then worsened again. However, over the course of the 20th century, the share of those from farm origins declined considerably. So, although farmers continued to be disadvantaged, the impact of their disadvantage on our overall measure of welfare was much reduced.

Taking figures 11, 12, and 13 together, we can suggest that the countries in our analysis fall into two broad groups: Britain, Sweden, and the Netherlands, where class inequalities in educational attainment were, and continue to be, relatively small; and Germany, France, Italy, and Ireland, where class inequalities were large in the oldest cohorts but have declined quite considerably during the 20th century. Poland also fits into this latter group, except that there the position of farmers is much worse than elsewhere. This grouping of countries is very similar to that found by Müller and Karle (1993) in their cross-sectional comparative analysis of educational inequality using CASMIN data. They find large inequalities in Germany, France, and Ireland and smaller ones in Sweden and the United Kingdom and a particularly disadvantaged position of farmers in Poland. In their analysis of social mobility in European countries, Breen and Luijkx (2004a, p. 60) find that the association between parental class and respondent's class is weakest in Sweden, Poland, and the Netherlands and strongest in Ireland, France, Italy, and Germany, with Britain occupying a position between these two groups. The degree of similarity (if not exact correspondence) between this grouping of countries based on their social fluidity and the groupings based on class inequalities in educational attainment is perhaps not surprising given the importance of education for occupational attainment.

## CONCLUSIONS

Social class disadvantages in children's educational careers have become less acute in the countries we studied, though this decline has been more pronounced in Sweden, the Netherlands, Britain, Germany, and France than in Italy, Ireland, and Poland. The rather unclear picture of the trend in inequality in Italy and Ireland may be due to the small sample size in these two cases. In Poland a decline in inequality in the older cohorts was followed by some reassertion of class origin effects in the younger cohorts, though this reassertion was very largely due to the worsening position of farmers and the self-employed. Overall, the decline in inequality that we



observe in our sample of countries took place, for the most part, during a relatively short period of around 30 years in the middle of the century, between the oldest cohort (born 1908–24) and the second youngest (born 1945–54 and thus in the educational system during the period 1950–75), though there is some variation in the timing of this decline. Consistent with our expectations, the decline in inequality was most evident and most widespread in the improved position of children from farming and working-class origins. Furthermore, these classes tended to shrink over the same period, so that as their disadvantage became less severe, it was suffered by a diminishing share of the population. In this way educational welfare, which we tried to capture in figure 13, increased. In the light of our findings, the *Persistent Inequality* thesis is evidently challenged, and the prevailing view that class inequalities in educational attainment will decline only under exceptional circumstances must be reconsidered. Putting the results from the Mare model alongside those from the ordered logit shows that one important mechanism for declining educational inequality in all countries was the substantial reduction in class origin effects at the transition to secondary education. In turn, this led to more equality in the attainment of higher educational levels even though inequalities in the transition to tertiary education remained unchanged.

In contrast to the collection of single-country studies in the Shavit and Blossfeld volume, our analyses were designed to ensure as much comparability across countries as possible. We used large samples drawn from many surveys whose quality was extensively checked; we used the same definitions of cohorts and, as far as possible, of class origins and educational attainment; and we applied the same statistical model in all countries. We sought to determine the extent to which our results might have been sensitive to choices of method, measures, and sample, and we found that they were largely unaffected by using parental education as an explanatory variable alongside class origins or by using the Mare model instead of the ordered logit. There is some evidence that change would have been less easy to detect if we had samples of the size used in the Shavit and Blossfeld volume. There the conclusion of no change in social origin effects on educational transitions was based on the statistical non-significance of the estimates of change rather than on the closeness of these estimates to zero (in fact, the relevant coefficient estimates in each chapter take a range of values showing no clear pattern). The effect of having a larger sample is not merely that it makes null hypotheses easier to reject, but also that it yields parameter estimates closer to their true value. Given our large samples, had inequality really been persistent, we would have expected to find coefficients for class origin effects that were similar in all cohorts. It may also be the case that our use of multiple surveys, as compared with the use of a single survey in most of the Shavit

and Blossfeld chapters, led to our estimates having less bias. Given survey-specific sources of error, multiple observations of the same cohorts over several surveys should lead to greater reliability of measurement.

Even given that the weight of evidence now supports the thesis of a declining association between class origins and educational attainment, it may be argued that to interpret this trend as demonstrating an increase in equality is mistaken because education is a positional good. In this case, the value of an educational qualification diminishes in proportion to the number of people who acquire it. But for this argument to have any force it is not enough to show that, over time, the value of some qualification declines; rather, it must be demonstrated that differences between the returns to educational levels diminish. The issue is not whether the returns to a tertiary qualification are less in one cohort than in an older one, but whether, for example, the gap in returns between a tertiary and an upper-secondary qualification has narrowed. As far as we know, there is no such evidence, and indeed, there are good grounds for supposing that, as the number of graduates increases, young people with only an upper-secondary qualification will be forced to take less attractive jobs than their counterparts in older cohorts.

A potentially more telling objection to the argument that declining association implies declining class inequality is that there may be distinctions within the broad CASMIN educational categories that are consequential for life chances. For example, it is commonly the case that differences exist between classes in their choice of particular subjects of study or field of education (Lucas 2001; Van de Werfhorst 2001; Kim and Kim 2003). If these differences have become stronger as inequalities in level of education have declined, then a focus solely on educational level will overestimate the extent to which inequalities have declined. Nevertheless, it is difficult to believe that inequalities stemming from differences in field of study within a given level of education will be as important for variations in life chances as differences between the levels of education attained.

The data that we have used are uniquely suited to the purpose of describing long-term trends in educational inequality and comparing these trends across countries. But this breadth of coverage in time and space comes at a cost: we lack many items of information that would be needed if we were to explain the trends. As a consequence, our article has been primarily empirical, descriptive, and methodological. But it is clear that our findings present a challenge to theory because much recent theorizing on educational differentials, such as Breen and Goldthorpe's (1997) relative risk aversion theory and Raftery and Hout's (1993) maximally maintained inequality hypothesis, has taken as its starting point the need to explain the supposed regularity exemplified by *Persistent Inequality*. Our

results show that the focus needs to shift not only from the explanation of stability to the explanation of change, but from accounts based on the assumption of widespread commonality to those that encompass the differences between countries in the magnitude of class inequalities in educational attainment and in the timing of their decline.

## APPENDIX

TABLE A1  
CASMIN EDUCATIONAL CATEGORIES

Category	Definition
1a .....	Inadequately completed elementary education
1b .....	Completed (compulsory) elementary education
1c .....	(Compulsory) elementary education and basic vocational qualification
2a .....	Secondary: intermediate vocational qualification or intermediate general qualification and vocational training
2b .....	Secondary: intermediate general qualification
2c_gen ...	Full general maturity qualification
2c_voc ...	Full vocational maturity certificate or general maturity certificate and vocational qualification
3a .....	Lower-tertiary education
3b .....	Higher-tertiary education

TABLE A2  
EGP CLASS CATEGORIES

Category	Definition
I .....	Higher-grade professionals, administrators, and officials; managers in large industrial establishments; large proprietors
II .....	Lower-grade professionals, administrators, and officials; higher-grade technicians; managers in small industrial establishments; supervisors of nonmanual employees
IIIa .....	Routine nonmanual employees, higher grade (administration and commerce)
IVa .....	Small proprietors, artisans, etc. with employees
IVb .....	Small proprietors, artisans, etc. without employees
IVc .....	Farmers and smallholders; other self-employed workers in primary production
V .....	Lower-grade technicians; supervisors of manual workers
VI .....	Skilled manual workers
VIIa .....	Semi- and unskilled manual workers (not in agriculture, etc.)
VIIb .....	Agricultural and other workers in primary production
IIIb .....	Routine nonmanual employees, lower grade (sales and services)

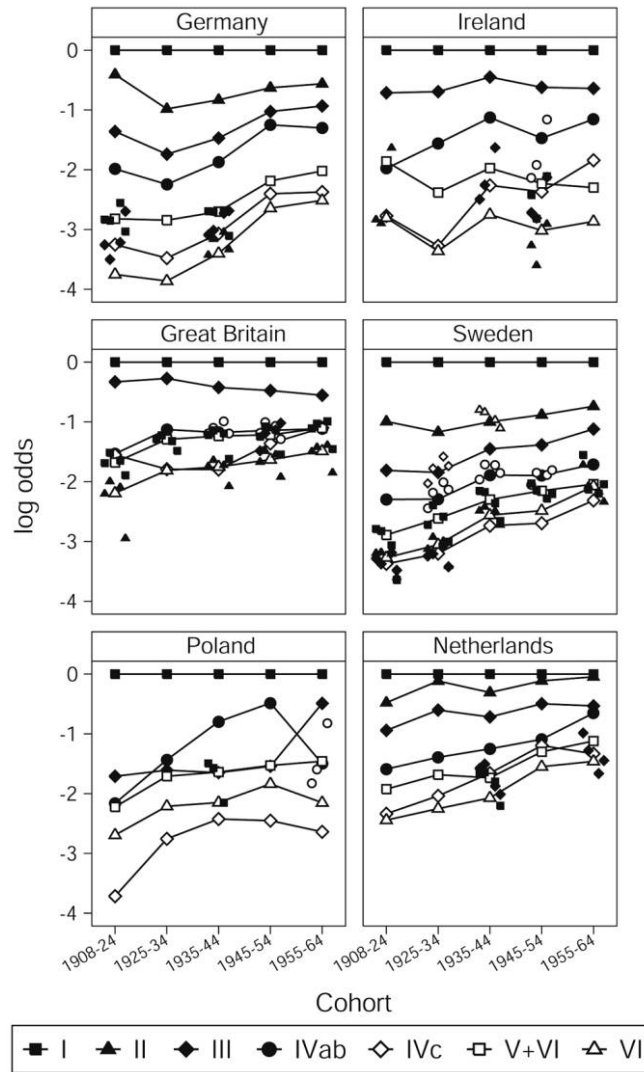


FIG. A1.—Ordered logit models for educational attainment; violations of proportional odds assumption displayed (cf. fig. 5). Class I is classes I+II in Ireland and Poland and classes I+II+IVa in Britain; educational levels 3a and 3b are merged in Italy, Ireland, and Poland; in Italy, the proportional odds assumption holds for all cohorts.

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